

directed to those minerals which are peculiar to Belgium.

Many of the figures will disappoint the modern reader on account of the indifferent printing; but among the illustrations he will find several useful diagrams which are not the familiar figures common to all the text-books, for example, the projection which shows the migration of the indicatrix axes with change of composition in the plagioclase felspars.

The authors have succeeded in producing within a small compass a fairly comprehensive yet lucid treatise on the principles of mineralogy and the chief mineral species, which may safely be recommended to the student in England as well as in Belgium.

The Essentials of Practical Bacteriology: an Elementary Laboratory Book for Students and Practitioners. By H. J. Curtis, B.S. and M.D., Lond., F.R.C.S. (London: Longmans, Green and Co., 1900.)

THIS book consists of a series of lessons upon practical bacteriology, mainly for a course of study required for the Diploma of Public Health. Commencing with the preparation of nutrient media, it passes on to the systematic study of, first, certain typical non-pathogenic bacteria, then to the moulds, including ringworm and allied forms, the account of which is much fuller than usual, and, lastly, to the pathogenic organisms. Fermentation and the beer yeasts are referred to, the malaria parasites, the *Amoeba coli*, and the supposed cancer organisms are described, and the methods employed for the examination of air, water, &c., and for testing disinfectants are given. The practical details described seem to be fairly complete and accurate, and the book is copiously illustrated, many of the illustrations of cultures being extremely good. The *Bacillus enteritidis sporogenes* of Klein is not mentioned, though it is a capital organism for class work. The method of freeing cultures for the "Widal" reaction from clumps by filtration is attributed to Symmers, but is mentioned in Hewlett's "Manual of Bacteriology." The paraffin method of embedding described is needlessly complicated. These and a few other omissions and errors will doubtless be corrected should another edition be called for.

What is Heat? and What is Electricity? By F. Hovenden. Pp. xvi + 329. (London: Chapman and Hall, Ltd., 1900.)

MR. HOVENDEN has set himself the modest task of overthrowing, in the space of about 300 pages, all existing physical tenets, and substituting in their place a remarkable theory of his own. In this effort he has not succeeded, except, apparently, to his own complete satisfaction. In the first part of the book the author quotes freely from Maxwell and others, and endeavours to prove that their reasoning is fallacious. His arguments only show that he does not understand what he quotes, and that he has not appreciated the most elementary principles of the subject, such, for example, as the difference between mass and weight. Having, as he considers, sufficiently disposed of the views held by modern men of science, Mr. Hovenden proceeds to the elucidation of his own theory. It is impossible to regard this part of the book seriously, Mr. Hovenden's deductions from experiments being altogether too extravagantly absurd. It is interesting to note that his treatment of the subject is throughout entirely qualitative; we venture to think that in no single instance would Mr. Hovenden's explanations stand the test of quantitative examination. If modern theory is to be disproved, it will not be by such writings as this. The least one can expect of its opponents is that they should properly understand the fundamental conceptions involved, and this Mr. Hovenden cannot be said to have attempted to do.

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LETTERS TO THE EDITOR.

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On a Proof of Traction-Elasticity of Liquids.

I HAVE read with much interest the note of Mr. T. J. Baker, on a surface-tension experiment (NATURE, No. 1600, June 28, 1900). The author describes, with photographic illustrations, a phenomenon at first observed by Savart (1833), and later studied by Hagen, Tyndall, J. Plateau, Boussinesq and myself, but in all these studies, as in Mr. Baker's note, no other force than surface tension is supposed to produce the different phases of the phenomenon. Therefore I resumed the subject two years ago¹ and endeavoured to explain the consecutive phases by proving that in this experiment there arises always some elasticity of traction, not only in both superficial layers, but even in the whole mass of the sheet.

For example, if the velocity of the jet is extremely high, the liquid is suddenly compressed by the shock against the disc; but on account of the perfect elasticity of the liquid, there is no sensible loss of *vis viva*, and the little expansion is performed in a very minute fraction of a second, during which the liquid is quickly projected in all directions parallel with the plane of the disc, and forms a sheet; as long as the intermolecular distances do not increase, the only retarding forces are the surface-tensions of both faces of the sheet; therefore the central part of the latter is even and transparent. But soon, by the stretching-out of the sheet, all molecules separate from each other, extremely little indeed, but enough to produce suddenly strong resistances; then each coming layer strikes against a retarded one, and so are formed circular strips from which many drops constantly part. Besides, as the elasticity cannot be the same in all points of a circular strip, some radial strips are also produced in the sheet, from whose broken edge very many little drops are continually thrown.

On diminishing the rate of outflow, the production of interior elasticity of traction becomes also smaller, and therefore the transparent portion of the sheet increases gradually; but the edge sinks slowly, and soon closes inwards and reaches the vertical piece supporting the disc. The surface-tension of both faces of the sheet is not the only force which drags in the water radially; for by the action of gravity the sheet can be compared with a membrane of india-rubber, that is to say, all portions are distended, not only in the superficial layers, but even in the interior mass.

It is easy to show that the distension of falling particles is all the greater as the velocity is smaller. Therefore the elasticity of traction produced by gravity increases in the proportion that the movement slackens.

We can now understand why the motion of the liquid in the vicinity of the summit of the closed figure becomes more and more difficult, until the figure rises above the plane of the disc, afterwards falls again and reforms a closed figure of smaller breadth.

With a still slower stream of water, the figure begins to oscillate vertically, just because the force of gravity draws it down, while the elastic force of traction pulls it up.

Ghent, January 2.

G. VAN DER MENSBRUGGE.

Mathematics and Biology.

IN the interesting address of Prof. Howes published in NATURE of December 10 occur the following words:—

"On this basis there are now being pushed forward attempts to apply statistical, experimental and mathematical tests to the study of vital phenomena. All honour to those who are making them, for it is certain there are phases of life capable of mathematical treatment, but the mystery of life can never be thus solved; and, concerning the objection to the observational method, with confirmation and generalisation, and rejection of the non-confirmable, our non-mathematical procedure is scientific. Huxley has long ago said of mathematics that what you get out of the machine depends entirely upon what you put into it."

¹ "Sur les nombreux effets de l'élasticité des liquides," 3^{me} Communication (Bull. de l'Acad. Roy. de Belgique, 3^{me} série, vol. xxxvi., p. 281, 1898.)

Now I think there are several points in the above sentences liable to misconstruction. Mathematics is purely a *form* of reasoning, and, as in the case of all forms of logic, it is merely an instrument, and the product depends upon the material dealt with. This may be the result of observation or of experiment, either of which may or may not be statistical in character. Prof. Howes, in contrasting "statistical, experimental and mathematical tests" with the "observational method," seems to be looking upon mathematical reasoning as something which has more relation to experiment than to observation. I fail to see why as an instrument it is less applicable to the gigantic over-thrust of the geologist than to the test-piece in the laboratory, less applicable to an observation on the mottling of birds' eggs than to an experiment on the breeding of mice. It is perfectly true, as Huxley said, that what you get out of the machine depends entirely on what you put into it. Such a platitude in its right context may be a useful reminder. But *without your machine you may be able to get nothing at all out of your material*; and I venture to think that this is the case, not with a few, but with many branches of biological inquiry.

The reason thereof is easy to find. In vital phenomena we are never able to repeatedly observe or to experiment, as we can very closely do in physics, under exactly the same conditions with the same quantities of the same substances. The reader will probably interject, "No, and this is the very reason why mathematics can be applied to the one and not to the other!" On the contrary, because in biological investigation an exact A cannot be associated with an exact B, and an exact C observed (as we can do in physics), biology requires a much more refined logic, much more subtle mathematics than the simplest branches, at any rate, of physical inquiry do. There is nothing more full of pitfalls than "ordinary reasoning" applied to the problems of association. The biologist observes that *some* A is associated with B, and that *some* C is associated with B. But if he wishes to discover whether the relation between A and C is causal, he will need all the refinements of symbolic logic, a mathematical analysis, which is analogous to the geometry of hyperspace, before he can come to a definite logical conclusion on the possible relationship of A and C. He may observe as much as he will, but he will not find out whether the association is confirmable or non confirmable without this higher logic. It is the all-pervading law of vital phenomena that no two individuals are identical among living forms, that variation exists in every organ and every character, which, so far from disqualifying biological phenomena for mathematical treatment, enforces a need for the most generalised forms of mathematical reasoning. Prof. Howes tells us that the mystery of life can never be solved by mathematical treatment. If he had said that the mystery of life cannot be solved by any treatment whatever, I should have heartily concurred with him. But if he means that observation, rather than observation plus the higher logic, is likely to discover the most comprehensive formula under which the phenomena of life can be described, then I am quite sure he is in error. Observation, for example, has collected a mass of most valuable facts during the past thirty years, but can any one by merely verbal generalising upon these facts venture to assert that evolution by natural selection is more than a probable hypothesis? The very nature of such ideas as variation, whether continuous or discontinuous, as inheritance, whether exclusive or blended, as selection, whether natural or sexual, leads us to the idea of number, of statistics, of frequency, of association, and enforces upon us an appeal to mathematical logic. If we are to feel that evolution by natural selection is as sure a formula as that of gravitation, it will be because mathematics steps in and reasons on the data provided by the Tycho Brahe's and Keplers of biological observation.

Prof. Howes must not for a moment suppose I claim biology for the mathematician. I do not even want the mathematician to have a biological training, conscious as I am personally of the disadvantages of its absence. The mathematician who turns physicist is rarely so valuable a discoverer as the born and trained physicist who knows mathematics so far as he needs them. I believe the day must come when the biologist will—without being a mathematician—not hesitate to use mathematical analysis when he requires it. The increasing amount of work being turned out, both in America and Germany, by the younger biologists with a mathematical training, is a sign of the times. In England, I suppose (where, as usual, an Englishman, Mr. Francis Galton, first indicated the great possibilities of a

new method), we shall be left behind, and let other nations gather the fruits of our sowing. Prof. Howes, indeed, leaves a field for mathematical investigation; but it was only a few weeks ago, at a discussion at the Royal Society, that another distinguished biologist asserted that in living forms there was no such thing as number!

Et Verbum interrogabat Vitam: Quod tibi nomen est? Et dicit ei: Legio, id est Numeri, mihi nomen est, quia multi sumus. Et deprecabatur eum multum, ne se expelleret extra regionem.

I doubt whether the demon can now be exorcised conjure *Verbum* ever so cunningly.

KARL PEARSON.

Education in Science.

SOME discussion has recently arisen as to the methods of teaching mathematics. Euclid has been condemned on the score of its advancement and its antiquity. An infusion of more modern geometry has been recommended, with corresponding arithmetic and algebra. In science, at the same time, there has been a tendency to recognise the historic method. Prof. Perry considers it unnecessary for pupils to traverse the course of their ancestors. But let us ask *why* this course has been recommended. On account of the successive growth of faculties in a historical sequence. Is this a fact or not? It is an undoubtable fact, and it is not sufficiently realised by any teachers. Prof. Perry has two saving principles, first to teach by practice, and second to satisfy the pupils' instincts. These being the same reasons which are used by advocates of historical methods secure a certain amount of agreement. We ought to arrive at the same result whether we study the natural methods of pupils, or the methods of primitive peoples. But Mr. Herbert Spencer has well pointed out somewhere that we ought not to go to the Greeks for examples of primitive peoples. They were highly and very specially developed. Hence arises a very great danger in the historic method.

With regard to practical and rational methods, it must have often been noticed by teachers that a great number of pupils have an inherent objection to carrying out rules without some kind of reason for them. It is also to be observed that a very vague, or even a verbal reason, will be more satisfactory than a real one. This is surely in accord with the studies of the history of science. Although it is somewhat misleading to reason from the experience of men of genius, it may be worth while to call to mind the intense satisfaction of Darwin with Euclid's concatenation, and the disgust of Huxley at the irrational rule and rote method of mathematics under which schoolboys grow up. It is the exceptional boy who delights in carrying out enigmatic rules, although all have a temporary taste for that work as sauce to the rest. It is treacherous to reason from one lesson of this kind to a regular course of it.

It is customary to speak of the activity, observation, ingenuity of children. But it is not found, either in the history of children or of primitive peoples, that they are capable of continued mental application, observation or contrivance. We might just as well speak of the great reasoning powers of children on account of their perpetual "why." It is also improper to underestimate the value of this tendency. By it children acquire and cement their knowledge, although a chain of real reasoning will absolutely exhaust them.

From this kind of reasoning we conclude that the time-taught method now pursued in mathematics is a reasonable one, that Euclid with the algebra and arithmetic corresponding is in the main advantageous. But why? Because it is conformable to the instincts of pupils, and also because it is historic. But is it conformable enough? Is it historic enough? I think not. Euclid was a grown man in a grown community of very special bent of mind. Where he does not agree with the reason for his inclusion in school curricula he should be neglected. But instead of supplementing him from more recent geometry, it should be from more antique writers and from study of pupils' methods.

Now we come to the bearing of this on the teaching of science. We are comparatively new at this game. We are finding that we have started in too high a key, and we are being recommended to go back. I have not yet seen a recommendation to exactly imitate the mathematical teachers, and go back to Pliny, Geber, Gilbert and Pallissy. But several have advised Boyle and Black. Along with this advice is an insistence on quantitative work from the very start. It appears to me this is a very grave mistake. The use of a rough balance and rough methods of measurement is all that should be aimed at in